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Article

Can Agricultural Interventions Improve Child Nutrition? Evidence from Tanzania

Anna Folke Larsen and Helene Bie Lilleør

Abstract

Severely reduced height-for-age due to undernutrition is widespread in young African children, with serious implications for their health and later economic productivity. It is primarily caused by growth faltering due to hunger spells in critical periods of early child development. We assess the impact on early childhood nutrition, measured as height-for-age, of an agricultural intervention that improved food security among smallholder farmers by providing them with a "basket" of new technology options. We find that height-for-age measures among children from participating households increased by about 0.9 standard deviations and the incidence of stunting among them decreased by about 18 percentage points.

JEL classification: I15, O13, Q16

Undernutrition is widespread and a key reason for poor child health in many developing countries. In Sub-Saharan Africa, around 40 percent of children under the age of five suffer from stunted growth, that is, severely reduced height-for-age relative to their growth potential (de Onis et al. 2011). Stunting is a result of periods of undernutrition in early childhood, and it has been found to have a series of adverse long-term effects in those who survive childhood. It is negatively associated with mental development (Martorell 1999), with human capital accumulation (Jamison 1986; Glewwe et al. 2001; Maluccio et al. 2009), with adult health (Victora et al. 2008; Adair et al. 2013), and with economic productivity and income levels in adulthood (Hoddinott et al. 2008, 2013).[1] Unfortunately, the evidence of how to reduce the prevalence of undernutrition among young children is somewhat mixed when it comes to typical nutrition programs, such as disease prevention strategies, breastfeeding practices, micronutrient supplements, and food fortification (Allen and Gillespie 2001; Bhutta et al. 2008; Schroeder 2008).

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1 Vogl (2014) shows that a sizeable fraction of higher adult wages may be mediated by occupational choice and better education.

Furthermore, there is virtually no rigorous evidence on the potential of agricultural interventions to reduce the prevalence of undernutrition among children (Masset et al. 2012; Ruel and Alderman 2013).[2]

The contribution of this article is a rigorous assessment of the impact on early childhood nutrition, measured as height-for-age, of an agricultural intervention that improved food security in the lean season among smallholder farmers in Northern Tanzania by providing them with a “basket” of new technology options (see Larsen and Lilleør 2014). Roughly half of the participating households had children under the age of five years.

Height-for-age is a strong biological marker of the nutritional status of children during the first 1,000 days of their lives, from conception to two years of age (Martorell 1999; Victora et al. 2008; Hoddinott et al. 2013). During this period, children have very high growth rates; and consequently, when subject to spells of growth faltering, children quickly fall behind the height-for-age growth curves of their peers, with limited chances of catching up subsequently (Victora et al. 2010).[3]

Using post-treatment data, we analyze whether the three-and-a-half-year-long agricultural intervention led to an improvement in the height-for-age measures among such young children. To identify the impact, we follow the identification strategy in Duflo (2003) and exploit the fact that height-for-age captures early-life undernutrition in the first 1,000 days, from conception to two years of age. We employ a difference-in-differences comparison of cohorts conceived before and after the phase-in of the project, where only the latter cohort lived all of their first 1,000 days under full project implementation. Under the assumption of a common growth profile for all children in absence of treatment, the height-for-age measures allow us to control for systematic differences in nutritional levels between older children in treatment and comparison households prior to the onset of intervention activities.

We find that young children from participating households on average experienced an improvement in their nutritional status, in that their standardized height-for-age measures increased by about 0.9 standard deviations. In addition, we find indications that stunting prevalence rates dropped by 17.6 percentage points. Compared to nutrition interventions, these are sizable impacts.[4] We show that improved food security in (severe) hunger periods is a probable mechanism behind this result. Our results are stable across numerous robustness checks. Furthermore, we find no evidence of time-varying differences, differences in fertility patterns or in drought coping strategies, all of which could potentially threaten our identifying assumption of a common growth profile.

I. The Agricultural Intervention

The agricultural intervention is called “Rural Initiatives for Participatory Agricultural Transformation,” or RIPAT.[5] The specific instance of this intervention that we evaluate was the first RIPAT program (RIPAT I), implemented by a local NGO, RECODA, in eight villages in Arumeru District in the Arusha Region of Northern Tanzania between 2006 and 2009 (see figure 1). The stated overall development goal of RIPAT is to reduce poverty and improve food security among smallholder farmers by facilitating high and sustainable levels of adoption of improved agricultural and livestock technologies disseminated

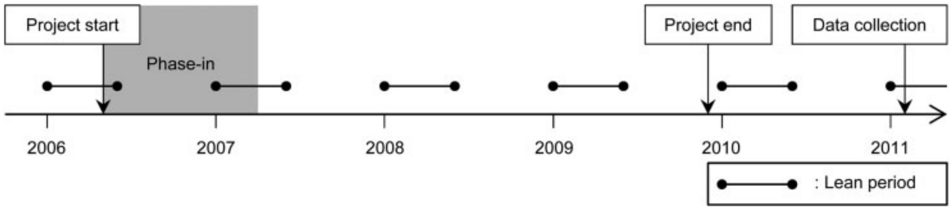
2 There are, however, studies of biofortification of crops and how that can improve the intake of different vitamins among children, e.g., Hotz et al. (2012a, 2012b) study the introduction of orange sweet potato on vitamin A intake among children in Uganda and Mozambique.

3 Although an opportunity window for catch-up may exist in the later puberty period, as recently shown by Hirvonen (2014).

4 Bhutta et al. (2008) report that the provision of food supplements in populations with insufficient food can increase the HAZ by 0.41 SD, while Caulfield et al. (1999) review efficacy trials to improve infant dietary intakes and find improvements in HAZ of 0.04–0.46 SD.

5 See http://www.ripat.org/home/ last accessed March 9, 2016 or Lilleør and Lund-Sørensen (2013) for a thorough description and discussion of the intervention.

Figure 1. Timeline



Source: RECODA and authors.

through local farmer groups. The intervention is similar to the Farmer Field Schools approach, the main differences are outlined in Aben et al. (2013).

Participation in RIPAT is not random. Poor villages with suitable agricultural conditions are selected at the district level. In the chosen villages, interested farmers (typically up to 70 in a village) are organized in farmer groups of 30–35 voluntary participants selected by the village council. In finding target participants, the village council is asked to select individuals who will be committed to the project (strict attendance records are kept), who are willing to share their new knowledge with fellow villagers, and who are not rich in terms of the internal village wealth ranking. However, to facilitate individual technology adoption, participants must own at least one acre (and no more than five acres) of farm land.

Once groups have been organized, facilitators from the implementing NGO meet with each group on a weekly basis during the phase-in period. Each farmer group rents or is allocated by the village council an appropriate group field of around one acre of land which can function as a demonstration plot. Here the group is offered training in a full basket of technology options. This reduces the individual risks involved in trying out or learning new technologies. The technology options include new banana cultivation techniques; new improved banana and other perennial and annual crop varieties; conservation agriculture for improved land utilization (such as minimum soil disturbance, cover crops, intercropping, rotation, and diversification of crops); post-harvesting technologies; improved animal husbandry; multipurpose trees for fodder, fruit, or firewood; soil and water conservation, including rain water harvesting; and savings groups. During the phase-in period of one year, the facilitators from the implementing NGO (typically agronomists) train the group members gradually in each of the technology options according to the agricultural seasons. After this period, the main role of the facilitators is to monitor and provide guidance on a bi-monthly or monthly basis.

Each farmer is free to choose which technologies to adopt on his/her own farm according to his/her own needs, constraints, and resources. Groups are given an initial set of necessary inputs for free for the training in, demonstration of, and testing of technologies on the group field only, while roosters of improved breeds are circulated among participating farmers to cross-breed with local hens. However, individual farmers wanting to adopt the new technologies must purchase inputs from the implementing NGO at cost prices. In the case of improved varieties of banana seedlings and goats, solidarity chains are implemented to promote local diffusion.[6] While some technologies are more popular than others, adoption varies considerably from farmer to farmer, and often takes place after a time lag.

In Arumeru District, food insecurity is pronounced in the months leading up to the annual harvest of the main staple crop, maize. The project implementation started in the beginning of the growing season

6 After the phase-in period and once banana seedlings are available from the group plot, the farmers can obtain free seedlings in exchange for agreeing to pass on three times the number of the seedlings received to other farmers within or outside the farmer group. The farmer tending a she-goat of an improved breed can keep the goat after passing on the first female offspring to another farmer on the same condition.

in 2006, and hence we would expect the earliest impact on food insecurity to have taken place in the lean season of 2007 (see figure 1).

II. Data and Summary Statistics

Our main outcome variable is the height-for-age z-score of children (HAZ), which we construct by subtracting the means and dividing by the standard deviations of the age- and gender-specific lengths or heights from the reference distribution established in the WHO Multicentre Growth Reference Study, which was based on healthy children from Brazil, Ghana, India, Norway, Oman, and United States (de Onis et al. 2004).[7]

We also look at the prevalence of stunting, using an indicator variable which equals one for those children whose height is less than two standard deviations below the age- and gender-specific mean.

Data

As indicated in the timeline in figure 1, we collected household-level data more than one year after the project was completed.[8] Based on the NGO records of participants, we traced and interviewed 506 of the 561 original RIPAT households from the eight intervention villages and 395 households from eight comparable nonintervention control villages in the same district, see table S1.1 in the online appendix (available at https://academic.oup.com/wber) for an overview of the sample composition and sources of attrition.[9] The comparison households were sampled at random among farming households with one to eight acres of land.[10] Out of these 901 households, 469 of them had children aged five years or less, in total 645 children. We are able to construct height-for-age z-scores for 482 children from 382 households. The main reason for attrition is that enumerators were not obliged to measure all children if some children were not present at the time of the interview.[11] The second most important reason for attrition is that not all parents knew the month of birth of their child, which is needed to find the relevant height from the WHO reference distribution. We disregard 14 child observations with missing values in the household characteristics and 11 child observations with an absolute HAZ larger than five standard deviations in order to avoid extreme outliers. Furthermore, following the convention in the literature (e.g., Bhutta et al. 2008; de Onis et al. 2011; Masset et al. 2012), we focus the analysis on children up to 60 months old, in order to avoid the influence of environmental factors on the heights of the children. This results in a final sample of 335 households with 396 children.

7 Though children below 24 months of age were measured recumbent, and hence we measured length rather than height, we henceforth refer to both length and height measurements as height.

8 In January 2011, we conducted a large scale quantitative household survey using a closed-form highly structured pilot-tested questionnaire to capture the impact of RIPAT on technology adoption, food security, and poverty. The data collection and data entry were closely supervised by us in cooperation with a survey management team from the Economic Development Initiative (a Tanzanian survey company). RECODA assisted in the hiring of a team of local interviewers and data entry clerks. Both the project implementation and the data collection were financed by the Rockwool Foundation.

9 Among the 55 households listed in the NGO records as participants but not traced and interviewed in the survey, half could not be identified and the other half had moved, died, or refused to participate in the survey.

10 During pilot testing of the survey, we became aware that some RIPAT participant did in fact hold more than five acres of land in 2011. To increase comparability, we therefore allowed households in comparison villages to to have up to eight acres of land. We control for land area in all the conditional estimations below and impose a restriction on the number of acres in the robustness section.

11 They were required to measure at least one child per household where there were children below six years of age.

In addition, we interviewed 427 nonparticipating households in RIPAT villages for a study of diffusion of improved banana cultivation using a stratified random sample (Larsen 2012).[12] From the households with young children we have HAZ measurements of 195 children, which we use in section V as an alternative comparison group.

Summary Statistics

In table 1, we list the mean values of key child, parent, household and village characteristics for the RIPAT households in column (1), and the corresponding values for the comparison households in column (2). In column (3), we present wild cluster bootstrap p-values from two-sided t-tests of whether the means differ between RIPAT and comparison households, clustered at the village level.[13] Corresponding numbers for the comparison group *within* RIPAT villages are shown in column (4) and (5).

Looking at the characteristics of children in the sample, we see that the overall HAZ is about one standard deviation below the WHO reference population mean, indicating that they suffer from undernutrition in general. One in four children are stunted, and although this might appear to be a high level of prevalence, it is well below the regional stunting prevalence rate of 44 percent as found in the 2010 Demographic and Health Survey (DHS 2010). This indicates that the children in our sample are somewhat better off than the regional average, possibly reflecting better socio-economic conditions, as the area is reasonably fertile and in close proximity to Arusha town.

Slightly more than half of our sample are girls, and most are children of the household head. Their fathers are typically in their late 30s, while their mothers are around 30 years old. Both parents have between six and seven years of schooling on average, corresponding to having almost completed primary education. However, there is a tendency for the parents in RIPAT households to be older and for the mothers to be slightly more educated than in both kinds of comparison households.[14]

The children live in households with, on average, five other household members, these being fairly evenly distributed across the four age groups shown. In 2006, prior to the commencement of the RIPAT project, the households owned on average three to four acres of land. The math skills of the farmers interviewed were tested through two simple math questions; less than half answered both of them correctly. We have also included the average historical rainfall level at the household level,[15] since the households mainly rely on rain-fed agriculture. In accordance with the village selection criteria of suitable agricultural conditions, RIPAT villages have received more rain than the comparison villages. Both RIPAT households and RIPAT villages are more likely to have participated in a development project in the past than their comparison equivalents. However, these differences are not statistically significant. The RIPAT villages are situated further away from the main local market and they are less likely to have a secondary school, and although these differences are insignificant they suggest that the program allocation procedure targeted wetter and more remote villages.

From table 1, it is thus clear that there are some differences in observables between participating and comparison households, although only few of these are significant at a conventional level. We return to

12 Nonparticipating households were therefore oversampled in villages with a larger degree of diffusion, and households growing improved bananas were sampled with a slightly higher probability than other households (see Larsen (2012) for details of the sampling scheme). We apply sampling weights to account for stratification.

13 We use wild cluster bootstrap-t *p*-values for all inferences in the paper because we only have 16 clusters (villages), and with few clusters the usual asymptotic theory does not apply (Cameron et al. 2008).

14 When we have not been able to identify the parents, we have imputed the sample mean following Duflo (2003).

15 We used interpolated data on yearly precipitation on a one-by-one kilometer grid measured in mm from the period 1950–2000 and available from http://www.worldclim.org/ last accessed March 9, 2016. The rainfall data were matched to households using GPS coordinates. While most households have adjacent plots, they may also have plots further away. In the estimations below, we control for rain based on household GPS coordinates, results are also fully robust to using village level averages instead.

Table 1. Summary Statistics

		(1) RIPAT	(2) Comparison	(3) *P*-value	(4) Within village	(5) *P*-value
Outcome variables	Height-for-Age Z-score	−0.94	−1.05	0.59	−1.25	0.26
		(1.66)	(1.66)		(1.48)	
	Stunting indicator	0.25	0.27	0.65	0.32	0.11
		(0.44)	(0.45)		(0.47)	
Child characteristics	Young indicator	0.61	0.65	0.26	0.53	0.07
		(0.49)	(0.48)		(0.50)	
	Age in months	34.11	31.20	0.11	36.35	0.11
		(15.36)	(15.52)		(14.49)	
	Girl	0.57	0.52	0.19	0.49	0.13
		(0.50)	(0.50)		(0.50)	
	Child of head	0.83	0.87	0.45	0.90	0.12
		(0.37)	(0.33)		(0.30)	
Parent characteristics	Father's education	6.78	6.53	0.25	6.62	0.28
		(1.68)	(1.67)		(1.65)	
	Father's age	39.12	36.99	0.02	36.38	0.07
		(8.10)	(8.25)		(7.62)	
	Mother's education	6.70	6.08	0.12	6.62	0.60
		(1.50)	(2.66)		(1.83)	
	Mother's age	31.85	28.67	0.00	29.65	0.03
		(7.17)	(6.70)		(6.74)	
Household characteristics	Household size	6.20	5.95	0.40	5.53	0.01
		(2.01)	(1.99)		(1.70)	
	HH members age 0–5	1.58	1.60	0.90	1.48	0.37
		(0.78)	(0.66)		(0.56)	
	HH members age 6–14	1.61	1.66	0.80	1.48	0.36
		(1.20)	(1.25)		(1.26)	
	HH members age 15–24	0.98	0.84	0.34	0.80	0.07
		(1.03)	(1.00)		(0.96)	
	HH members age 25–49	1.63	1.58	0.49	1.55	0.23
		(0.66)	(0.67)		(0.61)	
	Head is widow(er)	0.06	0.03	0.14	0.06	0.84
		(0.24)	(0.18)		(0.23)	
	Acres 2006	4.07	3.11	0.19	3.65	0.47
		(5.32)	(1.79)		(6.13)	
	Good in math	0.41	0.42	0.86	0.38	0.74
		(0.49)	(0.50)		(0.49)	
	Participation in other projects	0.27	0.16	0.14	0.13	0.09
		(0.44)	(0.37)		(0.34)	
	Household rain in mm	738.67	706.91	0.21	751.46	0.09
		(47.86)	(45.64)		(56.61)	
Village characteristics	Village distance to market	9.88	5.76	0.14		
		(3.90)	(5.00)			
	Village has secondary school	0.57	0.86	0.29		
		(0.50)	(0.35)			
	Village had devel. project	0.60	0.41	0.52		
		(0.49)	(0.49)			
Number of children		214		182	195	
Number of households		182		153	171	
Number of villages		8		8	8	

Notes: Variable means in samples of RIPAT children in column (1), comparison children in column (2), and children from non-RIPAT households within RIPAT villages in column (3). Standard deviations in parentheses. Column (3) and (5) gives wild cluster bootstrap-t *p*-values from two-sided t-tests of equal means of the RIPAT and comparison children from comparison villages and RIPAT villages, respectively, calculated as suggested by Cameron et al. (2008). Clustering is at the village level.

Source: Authors' calculations based on data described in text.

these below. It is, however, still important to account for these characteristics in the analyses below in order to increase comparability.

III. The Identification Strategy

The participation selection process at both village and individual levels suggests that more motivated farmers from poorer villages were likely to become project participants. Furthermore, no baseline data were collected prior to the intervention, and therefore we cannot rely on standard difference-in-differences estimates to establish counterfactual outcomes. To find an unbiased estimate of the average treatment effect of household participation in RIPAT on the nutritional status of children measured by their height-for-age z-scores (HAZ), we need to account for project placement and self-selection. We do so by employing the identification strategy of Duflo (2003).

This identification strategy relies on the findings in the medical literature that the *in-utero* period and the first two years of life are critical periods for childhood development. The length of newborn infants and the height of young children is considered to be more sensitive to the nutritional intake than the height of older children (Martorell and Habicht 1986; Martorell 1999; Ruel 2001), and stunting at birth or in early childhood is found to be a strong predictor of later childhood stunting (Adair 1999; Saleemi et al. 2001). Thus, because stunting is persistent, the HAZ of older children represents reliable recall data, as it is a biological marker of their past nutrition in early childhood (Victora et al. 2010; Hoddinott et al. 2013). We exploit this fact to identify the impact of RIPAT with a difference-in-differences estimator: the HAZ difference between young RIPAT and comparison children conceived after the phase-in of the project, net of the difference for the older children. The difference in height-for-age of the older children captures any systematic differences in nutritional status between RIPAT and comparison children before a potential impact of the project. That is, it captures nutritional-level differences due to the nonrandom selection and thereby accounts for the selection into the project.

In other words, the idea of the identification strategy is to estimate whether children who were conceived after project phase-in were taller for their age than their older peers who were conceived earlier, relative to a similar cohort difference between younger and older children from comparison households. The identifying assumption is that—in absence of treatment—the height-for-age of treated and comparison children would follow a common growth profile.[16] We capture a growth profile curvature by controlling for age in months quadratically. Our results could be misleading if the growth profiles differ between treated and comparison children in absence of treatment. We therefore also investigate whether there were any confounding time-varying differences between participating and comparison households, such as changes in fertility patterns or different coping abilities in times of drought (see section V).

We estimate the average treatment effect of RIPAT with ordinary least squares (OLS) using the specification in equation (1).

$$Y_i = \beta_1 RIPAT_h + \beta_2 young_i + \beta_3 RIPAT_h \cdot young_i + C_i\delta + P_i\phi + X_h\eta + W_v\gamma + \varepsilon_i \tag{1}$$

Y_i is the outcome for child i in household h in village v. The variable $RIPAT_h$ indicates whether household h had ever participated (i.e. including those that dropped out) in a RIPAT farmers' group; $young_i$ indicates whether child i was younger than a certain threshold described below; and $RIPAT_h \cdot young_i$ gives the interaction between the two last variables. Thus, β_3 will give the estimate of the average treatment effect of RIPAT on the nutritional status of young children, net of selection. We control for child characteristics, denoted as C_i, parent characteristics, P_i, household characteristics, X_h, and village characteristics, W_v, all of which are listed in table 1. Age in months is included quadratically. We take the logarithm of acres of land owned in 2006. Finally, we allow for errors to be correlated within villages, $\varepsilon_{i,v}$.

16 This corresponds to the common trends assumption in a classical difference-in-differences set-up.

We have a small subsample of households with measurements of both young and older siblings. This allows us to also provide estimates with household fixed effects instead of parent, household, and village characteristics as a simple robustness check.

There is some flexibility in how we define the relevant threshold for the *young* dummy, as it depends on when we can expect an impact of RIPAT on food security to have taken place in the households. Food insecurity in this area is highly seasonal, and is only pronounced in the lean seasons (January to May).[17] This implies that the earliest point in time, where we can expect an impact on nutrition of pregnant women and young children is in the first lean season after project start, January–May in 2007. Hence, we define the *young* dummy to be equal to one for children conceived in January 2007 or later (henceforth referred to as "young" children).[18] Regardless of the choice of threshold, some children classified as *old* may also be affected by the improved nutrition. If there is any such catch-up growth, it will lead to an underestimation of the impact. We examine the choice of threshold in the online appendix S2.2.

IV. Results

Before turning to the estimation results we compare the distributions of HAZ presented in figure 2 for the *old* and *young* children separately. We have conditioned on child, parent, household, and village characteristics to reduce noise. We see that the conditional distribution of HAZ for the *old* RIPAT children is closely aligned to that of *old* comparison children, suggesting that these children are indeed highly comparable. For the *young* children the RIPAT distribution is clearly shifted to the right of the comparison distribution. Although this graphical inspection does not constitute a formal test, it does suggest that not only were the *young* RIPAT children taller for their age than the comparison children *on average*, but it appears that the intervention has affected the entire HAZ distribution of *young* RIPAT children, in particular the lower tail.

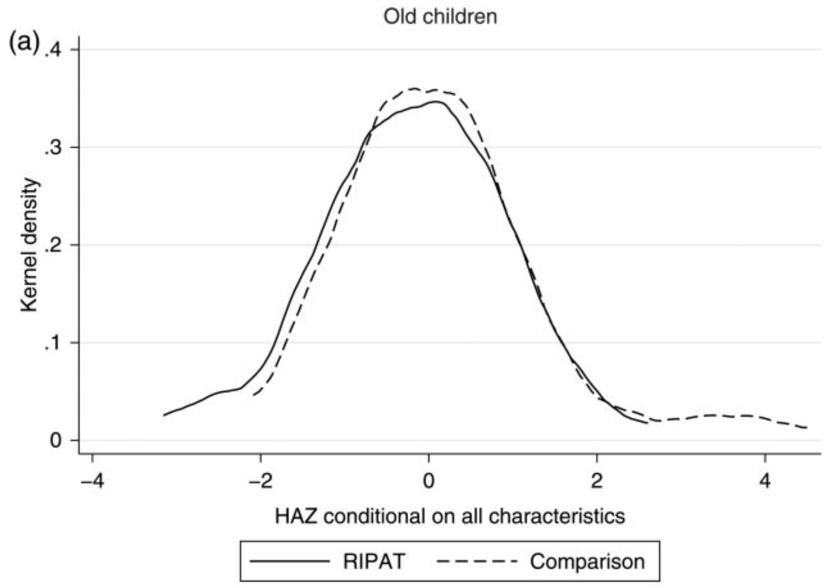
OLS estimation results are shown for the average treatment effect of RIPAT using the econometric specification given in equation (1) in table 2. Columns (1) to (3) present estimated impacts on the height-for-age z-score (HAZ) of *young* children in participating households, hence the impact on the mean value of the HAZ distribution, while column (4) provides the linear probability estimates for the impact on the likelihood of children being stunted. The coefficient to the *RIPAT* and *young* interaction term gives an estimate of the average treatment effect of RIPAT on the HAZ or the probability of being stunted among the younger children who grew up under the influence of RIPAT. In column (1) we show the unconditional estimates, in column (2) we control for child, parent, household, and village characteristics, and in column (3) we allow for household fixed effects only using the subsample with both *old* and *young* children in the household.

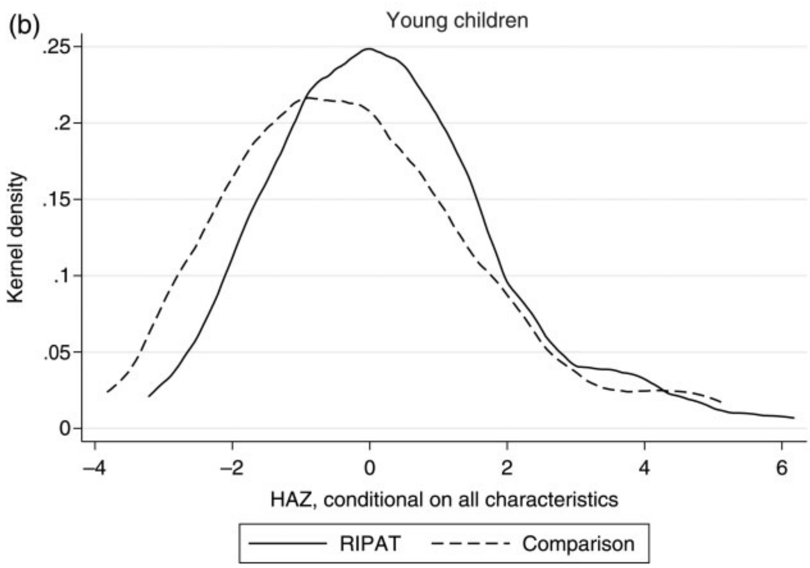
The unconditional estimate of the impact of RIPAT on HAZ is an average improvement of 0.57 standard deviations (SD) of the WHO reference distribution. When we control for child, parent, household, and village characteristics, the estimate of the impact increases to 0.88 SD. This means that young children in RIPAT households were 0.88 SD taller than their peers in comparison households, controlling for any pre-project differences among the older children. When we include household fixed effects to account for unobserved household characteristics, the point estimate further increases to 1.38 SD.

17 We define the span of the lean season according to self-assessment by the households in the sample. The majority of households mentioned the months January–May as part of the "worst period in terms of having enough food for everyone in your household [during 2010]".

18 We calculate month of conception to be nine months before month of birth. If RIPAT reduces prematurity rates *young* comparison children are on average conceived later than *young* RIPAT children. However, increasing the *young* threshold for comparison children one or two months does not alter the results. Furthermore, there is a possibility that RIPAT has improved the survival rate of weak fetuses and infants in which case we will most likely underestimate the impact. Unfortunately, we do not have mortality data to test this hypothesis.

Figure 2. Distributions of the HAZ





Note: Kernel densities of residuals from the regression of HAZ on all individual, parent, household and village characteristics as described in text.
Source: Authors' analysis based on data described in text.

The fact that we still find a positive impact after the introduction of household fixed effects suggest that the results are not driven by unobserved differences in the selection into the project between households with young and older children.[19]

19 It should be noted that our fixed effect estimation relies on variation in a relatively small subset of the sample, as only 21 RIPAT households and 19 comparison households had both young and older children in the sample. We therefore only include it as a robustness check of the conditional estimates.

Table 2. Impact of RIPAT on HAZ

	HAZ			Stunting
	(1)	(2)	(3)	(4)
RIPAT and young	0.569*	0.879 ***	1.377 **	−0.176 *
	(0.29)	(0.29)	(0.46)	(0.09)
	[0.062]	[0.012]	[0.002]	[0.094]
RIPAT	−0.240	−0.215		0.090
	(0.20)	(0.24)		(0.06)
Young	−0.025	−0.133	−1.279	0.060
	(0.11)	(0.30)	(0.87)	(0.09)
Child characteristics	No	Yes	Yes	Yes
Other characteristics	No	Yes	No	Yes
Household fixed effects	No	No	Yes	No
Clusters (villages)	16	16	13	16
Observations	396	396	86	396

Notes: OLS estimates with HAZ as dependent variable, cluster standard errors in parentheses, and wild cluster bootstrap-t *p*-values in square brackets. "Other characteristics" include parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Source: Authors' analysis based on data described in text.

Because RIPAT is a village intervention, we cluster standard errors at the village level, and the corresponding significance levels are reported with the customary use of asterisks. Since we only have 16 villages and thus 16 clusters, the standard asymptotic theory cannot be applied for inference and we report *p*-values in square brackets based on wild cluster bootstrapped t-statistics for the impact coefficients, as suggested by Cameron et al. (2008).

Turning to the impact on stunting in column (4), we see that the average impact on height-for-age also translates into an impact among children suffering from severe malnutrition. Compared to children in comparison villages, we find that *young* RIPAT children experienced a reduction in the prevalence of stunting of 17.6 percentage points, significant at the ten percent level. We have less statistical power compared to our results for HAZ, since we discard information by reducing the continuous HAZ to a binary variable.[20]

When we measure the impact of RIPAT on HAZ, we measure the impact on a nutritional stock (height). We expect RIPAT to affect the stock through improvements in the nutritional flows. This suggests that the effect of RIPAT on height-for-age should increase with the duration of exposure to RIPAT. The longer children were exposed to improved nutrition, the more the impact accumulates in their stock, that is, their height. However, the agricultural intervention was gradually phased in, and there is a natural lag from the introduction of new technologies to a tangible nutritional outcome among participating households. Children born early in the project period therefore received a weaker nutritional improvement during their first 1,000 days than children born later. This works in the opposite direction.

In table 3 we therefore present estimates from a model that allow for cohort-specific impacts: instead of a *young* indicator we include age indicators for the years zero to three, along with the RIPAT indicator and their interaction terms. Four-year-old comparison children form the reference group. Overall, the impact is driven by the one- and two-year-olds, both groups experience impacts of 1 SD (see column 2), suggesting that the expected accumulation in nutritional stock among the two-year-olds is offset by the gradual phase-in of the project. The impact among the youngest children is not statistically significant and as expected, there is no significant difference between the older three- and four-year-old RIPAT

20 We find these effects to be homogeneous across boys and girls. Results are available upon request.

Table 3. Cohort Specific Impacts on HAZ

	(1)	(2)
RIPAT and age 0	−0.237	0.274
	(0.65)	(0.67)
	[0.666]	[0.700]
RIPAT and age 1	0.666	1.097*
	(0.49)	(0.53)
	[0.198]	[0.064]
RIPAT and age 2	0.473	1.012**
	(0.46)	(0.40)
	[0.282]	[0.018]
RIPAT and age 3	−0.332	0.106
	(0.34)	(0.43)
	[0.388]	[0.786]
RIPAT	−0.042	−0.179
	(0.29)	(0.35)
Age 0	0.681	−0.366
	(0.52)	(1.37)
Age 1	−0.096	−0.751
	(0.30)	(0.88)
Age 2	−0.200	−0.663
	(0.30)	(0.54)
Age 3	0.107	−0.102
	(0.31)	(0.44)
All characteristics	No	Yes
Clusters	16	16
Observations	396	396

Notes: OLS estimates with HAZ as dependent variable, cluster standard errors in parentheses, and wild cluster bootstrap-t *p*-values in square brackets. "All characteristics" includes child, parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Source: Authors' analysis based on data described in text.

cohorts relative to the comparison cohorts.[21] The latter result thus supports our common growth profile assumption for the treatment and comparison group prior to any impact.[22]

Robustness Checks

In the online appendix S2, we examine whether the results above might be driven by systematic errors or decisions concerning the data. We find that our results are robust to accounting for attrition, to different thresholds of the *young* indicator, and to changing the sample selection with respect to children's age, with respect to number of acres owned, with respect to outliers and to data quality considerations in terms of correct measurement procedures and formal registration of child age. The estimated impacts of RIPAT on height-for-age range from 0.6 to 1.2 standard deviations, and all but one of them are significant at the ten percent level. We also find consistent large impacts of participation in RIPAT on the prevalence of stunting, but at reduced power levels.

21 The *young* threshold is 39 months, i.e., three years and three months, so 22 of the 90 three-year-old children are considered *young* in the main analysis.

22 The same age pattern is found if we use alternative functional form specifications such as 12-month splines or allowing the effect to depend quadratically on age in months.

Mechanisms

We cannot pin down the exact channel through which RIPAT has influenced the nutritional status of young children, but we can examine the most likely chain of events, namely, whether the results of increased technology adoption and improved food security found in Larsen and Lilleør (2014) for the full sample of households, also holds for this sub-sample of RIPAT households with young children, using simple linear regression comparisons.[23] RIPAT households are significantly more likely to grow improved banana varieties, and to keep improved breeds of chickens and goats (see Panel A of table 4). In the online Appendix S3, we further document high rates of adoption for the other introduced technologies.

Table 4. Adoption of Technologies and Food Security

	(1) RIPAT	(2) Comparison	(3) Cond. difference
Panel A: Adoption of technologies			
Improved banana cultivation	0.657	0.121	0.523***
	(0.476)	(0.327)	(0.103)
			[0.030]
Improved breed of poultry	0.309	0.013	0.243***
	(0.463)	(0.115)	(0.055)
			[0.032]
Improved breed of goats	0.354	0.128	0.227***
	(0.480)	(0.335)	(0.044)
			[0.006]
Panel B: Food security			
Number of worst months	3.831	4.150	−0.438***
	(1.338)	(1.445)	(0.135)
			[0.038]
No hunger	0.365	0.265	0.159***
	(0.483)	(0.443)	(0.050)
			[0.036]
Meat consumption last week	0.764	0.694	0.183**
	(0.426)	(0.462)	(0.085)
			[0.192]
Egg consumption last week	0.607	0.408	0.152**
	(0.490)	(0.493)	(0.068)
			[0.180]
Dairy consumption last week	0.843	0.810	0.086
	(0.365)	(0.394)	(0.128)
			[0.664]
Number of households	178	149	327

Notes: Variable means in samples of RIPAT and comparison children and standard deviations in parentheses in columns (1) and (2). Column (3) presents OLS estimates from regressions of the technology or food security variable on a RIPAT indicator, cluster standard errors are in parentheses, and wild cluster bootstrap-t p-values are in square brackets. Regressions also control for education and age of the household head and household and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Source: Authors' analysis based on data described in text.

23 In Larsen and Lilleør (2014), we estimate the average treatment effect using simple cross-sectional comparisons between treatment and control groups, matching estimators, and a difference-in-differences estimator exploiting the gradual roll-out. The findings are reasonably robust across estimation methods, suggesting that selection into the project is not a major driver of results. We are therefore confident that when we employ simple cross-sectional comparisons to this subsample, it will give a good indication of whether there was also increased adoption and improved food security levels in the subsample of RIPAT households with young children.

The adoption of both perennial crops (like banana) and improved livestock technologies (poultry providing eggs and meat, and milking-goats providing milk) is likely to enhance production smoothing over the agricultural cycle which in turn facilitates smoothing of food consumption over the year. RIPAT households have a significantly shorter hunger season as can be seen from Panel B of table 4, just as they are significantly less likely than comparison households to have experienced any hunger during the 12 months before the interview.[24] In addition, RIPAT households are significantly more likely to have had meat and egg during the last week before the interview.[25] Hence, increased consumption of animal-source foods is also a potential pathway to improved child height-for-age since animal products contain nutrients that are important for child linear growth (Bhutta et al. 2013). Unfortunately, lack of data on changes in the dietary intakes of children prevents us from further examining this channel.

This suggests that the positive impact on the height-for-age of young RIPAT children is likely to come about through higher levels of technology adoption promoting higher levels of food security in the lean season of the year and a larger intake of animal-source foods. Not being exposed to hunger spells seems to have long-lasting consequences for the growth curves of these young children. The effect may be reinforced by less exposure to fecal bacteria,[26] which could also reduce the prevalence of stunted growth (Humphrey 2009).

Finally, we examined whether RIPAT households had lower poverty levels than the comparison households, but find no clear evidence of such differences (see online Appendix S5).

V. Possible Alternative Explanations

Our identification strategy relies on the standard assumption of a common growth profile in the absence of treatment. We study three potential factors that could violate this assumption; time-varying differences between RIPAT and comparison villages, differences in fertility patterns between RIPAT and comparison households, and differences in households' coping capabilities in times of drought.

Village Differences

If the RIPAT and comparison villages were differentially exposed to shocks, our impact estimates may be confounded. In fact, the area was hit by a severe drought in 2009. We therefore also compare RIPAT children to children *within* the RIPAT villages who did not live in participating households, although results may be biased due to technology diffusion within RIPAT villages (Gausset and Larsen 2013). We use children from non-RIPAT households in RIPAT villages as a comparison with an additional column allowing for village fixed effects in table 5, which corresponds to table 2.[27]

The estimated impact on HAZ is much in the same order of magnitude as in table 2, but the small number of clusters affects the bootstrapped p-values and we have less power. Since the within-village comparison yields similar results to the comparison across villages we can rule out the possibility that the estimates are driven purely by differences in village-level shocks.

24 Additional measures of food security are presented in the online appendix S4.

25 Though only at the 20 percent level when accounting for the low number of clusters.

26 Zero-grazing among livestock meant keeping animals in small enclosures, this reduces the exposure of young children to animal excrement. In addition, RIPAT households are more likely to have a roofed pit-latrine (RIPAT facilitators recommended following such government regulations), this would have reduced the spread of bacteria through flies.

27 Standard errors are again clustered at village level (note that now there are only eight villages) and p-values based on wild cluster bootstrapped t-statistics are shown in square brackets.

Table 5. Impact on HAZ and Likelihood of Stunting with Weighted RIPAT Village Comparison Sample

	HAZ				Stunting
	(1)	(2)	(3)	(4)	(5)
RIPAT and young	0.832*	0.788	0.592	0.834*	−0.267**
	(0.43)	(0.43)	(0.41)	(0.44)	(0.09)
	[0.104]	[0.138]	[0.272]	[0.474]	[0.056]
RIPAT	−0.129	−0.226		−0.257	0.083*
	(0.31)	(0.28)		(0.31)	(0.04)
Young	−0.287	−0.547	−1.917	−0.550	0.185
	(0.31)	(0.35)	(1.65)	(0.35)	(0.16)
Child characteristics	No	Yes	Yes	Yes	Yes
Household characteristics	No	Yes	No	Yes	Yes
Village characteristics	No	Yes	No	No	Yes
Fixed effects	No	No	Household	Village	No
Clusters (villages)	8	8	8	8	8
Observations	409	409	85	409	409

Notes: OLS estimates using a comparison sample within RIPAT villages weighted with inverse sampling probabilities. Column headings refer to the dependent variable. In parentheses are cluster standard errors, and in square brackets are wild cluster bootstrap-t *p*-values. "Household characteristics" includes parental characteristics. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Source: Authors' analysis based on data described in text.

Fertility Patterns

The estimated impact would be confounded if project participation itself lead to endogenous changes in fertility patterns and thus in cohort composition among the participating households relative to comparison households.

First, if the intervention induces households to have fewer children, the households would have more resources per child, which could have led to an improvement in the nutritional status of the children born. However, since we control for the number of household members between zero and five years of age, this is unlikely to be driving the impact we find. It could also lead to an increase in birth spacing, but we do not find any difference in birth spacing between RIPAT and control children.

Second, if participation in the project changed the timing of fertility, this could potentially affect the group composition of *old* and *young* RIPAT children *vis-a-vis* the comparison children. Table 1 shows that the group of RIPAT children were on average slightly older (three months) than the group of comparison children, although not significantly. We further test the composition of the age cohorts by regressing age indicators on a RIPAT indicator, while controlling for household and village characteristics (see figure S6.1 in the online appendix). We find no significant differences in the age composition of the RIPAT and comparison sample.

Third, if the project affected timing of conception over the year, RIPAT children might have been differently exposed to the lean season relative to the comparison children, which again could affect our results. Hence, we run twelve regressions with month of birth indicators as dependent variables using the same specification as in equation 1 (see figure S6.2 in online appendix).[28] With this difference-in-differences specification we test whether there has been a shift in the seasonal timing of fertility from the *old* to the *young* RIPAT children which is different from any potential shift over time for the comparison children. Out of the twelve tests, the only significant difference we find is that *young* RIPAT children are less likely than *old* RIPAT children to be born in November relative to any difference between the *young* and *old* comparison children. For this difference to be driving our results it would need to be very

28 All children, household and village characteristics are included except the child's age.

unfavorable to be born in November as compared to other months of the year. Our results are robust to excluding children born in November (results available upon request).

Capabilities for Coping with Drought

Finally, the common growth profile assumption could also be violated if the RIPAT and comparison households had coped with the 2009 common shock in different ways, regardless of project participation. Although RIPAT aims to reduce vulnerability to drought shocks by introducing drought-resistant crops and production-smoothing technologies, we need to address the concern that households who selected into RIPAT may *initially* have had different coping strategies than the comparison households. To do so, we investigate whether the impact is driven by any of the *observed* differences in parent and household characteristics. Table 1 shows that parent characteristics differ significantly between RIPAT and comparison households in terms of father's and mother's age. Furthermore, mother's education, which is often a strong predictor of childrens' health, is also marginally different, with children in RIPAT households having more educated mothers. If, say, older or better-educated mothers were better at nourishing their children during the 2009 drought, we would overestimate the impact, since RIPAT mothers were on average better educated.

We demean these key parental variables and interact them with the *young* indicator, the *RIPAT* indicator and their joint interaction term to allow for the treatment effect to depend on, for example, mother's age. The estimation results are given in columns (1)–(3) of table 6. The estimates of the impact of RIPAT at the mean values of the parent characteristics are remarkably stable, confirming that the impact found above is not driven by any of the differences in observed parental characteristics.

Table 6. Heterogeneous Impacts on HAZ

Q:	(1) Father's age	(2) Mother's education	(3) Mother's age	(4) Log acres 2006	(5) Participated in prior project(s)	(6) Historical rainfall
RIPAT and young	0.796**	0.892***	0.781**	0.901***	0.832**	0.773***
	(0.279)	(0.273)	(0.287)	(0.261)	(0.296)	(0.240)
	[0.018]	[0.004]	[0.032]	[0.006]	[0.026]	[0.016]
RIPAT, young and Q	−0.078***	0.015	−0.024	−0.379	−0.749	0.012***
	(0.025)	(0.127)	(0.044)	(0.458)	(0.595)	(0.003)
	[0.024]	[0.902]	[0.544]	[0.394]	[0.286]	[0.014]
RIPAT	−0.196	−0.219	−0.177	−0.232	−0.170	−0.128
	(0.241)	(0.239)	(0.216)	(0.235)	(0.219)	(0.194)
RIPAT and Q	0.024	−0.123	−0.026	0.163	0.958**	−0.012**
	(0.019)	(0.080)	(0.029)	(0.278)	(0.431)	(0.005)
Young	−0.077	−0.173	−0.069	−0.133	−0.066	−0.163
	(0.318)	(0.289)	(0.311)	(0.280)	(0.290)	(0.301)
Young and Q	0.065***	0.026	0.027	−0.004	0.253	−0.002
	(0.014)	(0.049)	(0.035)	(0.420)	(0.312)	(0.002)
Q (not demeaned)	−0.051**	0.004	0.034	0.003	−0.325*	0.003
	(0.019)	(0.057)	(0.027)	(0.225)	(0.179)	(0.003)
All characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (villages)	16	16	16	16	16	16
Observations	396	396	396	396	396	396

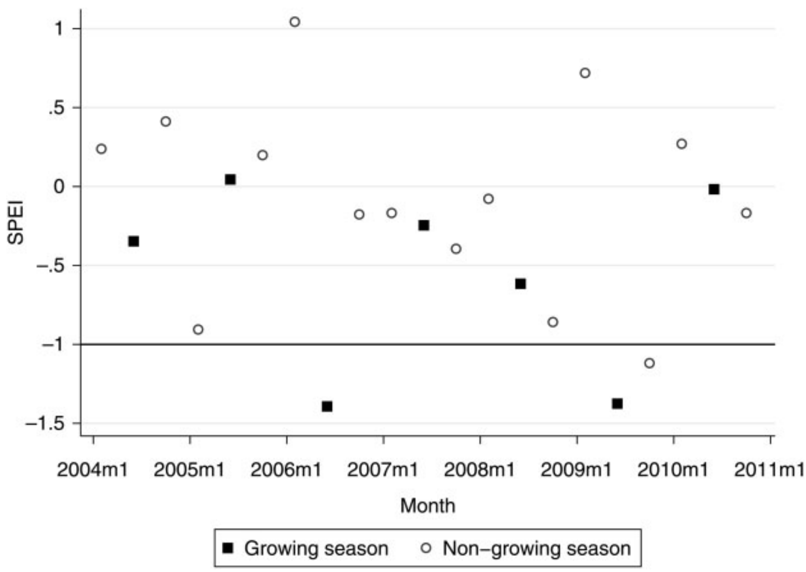
Notes: OLS estimates, cluster standard errors in parentheses, and wild cluster bootstrap-t *p*-values in square brackets. Q refers to the variable stated in the column heading; the variable is demeaned when it enters an interaction term, but not when included in levels. "All characteristics" includes child, parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Source: Authors' analysis based on data described in text.

Second, using the same method, we examine whether household self-selection into the project and the land ownership criteria could be driving the results. We proxy self-selection by using participation in other projects in the past. From table 6 columns (4)–(5), we see that differences in land ownership or prior participation in other projects do not alter the estimated impact of RIPAT on the HAZ of *young* children. In addition, we check the role of rainfall. Villages were partly chosen based on suitable agricultural conditions, including sufficient rainfall. We do find that part of the impact of RIPAT on HAZ is driven by a positive interaction with rainfall, but the effect of RIPAT at the mean rainfall level is still 0.77 SD.

Finally, we check for intrinsic *unobserved* differences in strategies for coping with shocks between participating and comparison households by comparing the HAZ of children exposed to a drought spell in 2006, which was prior to any nutritional impact of RIPAT activities.[29] Standardized Precipitation and Evapotranspiration Indices (SPEI)s for the period 2004 to 2011 with three data points per year are shown in figure 3. It can clearly be seen that the growing seasons of 2006 and 2009 were particularly dry.[30]

Figure 3. Standardized Precipitation and Evapotranspiration Index



Source: The global SPEI database, http://sac.csic.es/spei/database.html (version: 2.0) last accessed March 9, 2016.

If RIPAT and comparison households initially had different drought coping strategies, we should expect to see differences in the HAZ of children conceived just before or during 2006. These are precisely the children we define as *old*, and where we find no significant difference in their height-for-age between RIPAT and comparison children. This renders it unlikely that the improved nutrition among the young

29 To measure weather shocks, we follow Harari and La Ferrara (2013) and examine monthly Standardized Precipitation and Evapotranspiration Indices (SPEIs) for the geographical area under study, using the average of the four preceding months and considering values of the SPEI below one SD as negative climate shocks. We consider March to June to be the main growing season based on the Food and Agriculture Organization crop calendar, http://www.fao.org/agriculture/seed/cropcalendar/welcome.do last accessed March 9, 2016.

30 The graph is from a grid covering half of the villages in our sample; the graph from the neighboring grid covering the remaining villages is very similar and is available from the authors. The global SPEI database can be found at http://sac.csic.es/spei/database.html last accessed March 9, 2016.

RIPAT children is driven by differences in drought coping strategies across treated and comparison households *a priori*.

It is, however, very likely that RIPAT farmers improved their ability to cope with the 2009 drought through the adoption of drought-resistant crops and production-smoothing technologies. The magnitude of our estimated average treatment effect on HAZ might therefore have been considerably smaller if the area had experienced years of bumper harvest and thus little food insecurity and no hunger spells prior to the survey.

VI. Discussion

Given the widespread prevalence of stunted growth and the relatively recent acknowledgment of its many long-term adverse implications, combating undernutrition of unborn and infant children has become an important subject that attracts attention from both researchers and policy makers; see, for example the recent Lancet reviews by Bhutta et al. (2008); Victora et al. (2008); Ruel and Alderman (2013); and the Cost Of Hunger in Africa report by African Union Commission et al. (2014). However, there is lack of rigorous evidence when it comes to the scope for agricultural interventions to combat stunting and underweight among young children (Masset et al. 2012).

We find that the agricultural intervention, RIPAT, has improved drought resilience among the participating farmers in northern Tanzania by introducing a basket of technology options based on local resources including crop diversification, perennial crops, conservation agriculture, improved animal husbandry, and land use management. This holistic approach may have been key in improving the nutritional status of young children in the participating households, these components help to improve the nutritional quality of farming output according to Miller and Welch (2013). We find that the RIPAT intervention had a significant positive impact of about 0.9 SD on the height-for-age z-scores of young children who had been fully exposed to the project in their early life. Similarly, we see a reduction in stunting prevalence among the *young* group of RIPAT children of around 18 percentage points.

There are two important points to note concerning these impacts. First, they were measured almost five years after the start of the project, which lasted three and a half years, suggesting that these are sustainable impacts, but not necessarily quick impacts. Second, toward the end of the project implementation period, a serious drought hit the area, worsening and lengthening the annual hunger period. This has possibly increased the difference in undernutrition levels found between participating and comparison households, since the intervention was designed to increase the drought resilience of farmers and shield their food production, rather than to boost agricultural output during bumper years. This is important to keep in mind, as it influences the external validity of these results relative to other areas, for example, areas less prone to undernutrition, or even to the same area in bumper years.

There are reasons to believe that precisely because of the holistic nature of the intervention and its focus on shielding farmers' food production against adverse impacts of drought, the nutritional and thus growth impacts on young children are sizable and larger than those typically found in more narrow nutrition interventions as reviewed in Bhutta et al. (2008) and Caulfield et al. (1999). As hypothesized by both Masset et al. (2012) and Ruel and Alderman (2013), our study confirms that there is scope for agricultural interventions in alleviating undernutrition and that they can indeed be very effective.

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